Event Studies and the Law: Part I: Technique and Corporate Litigation

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Event studies are among the most successful uses of econometrics in policy analysis. By providing an anchor for measuring the impact of events on investor wealth, the methodology offers a fruitful means for evaluating the welfare implications of private and government actions. This article is the first in a set of two that review the use and impact of the event study methodology in the legal domain. This article begins by briefly reviewing the event study methodology and its strengths and limitations for policy analysis. It then reviews in detail how event studies have been used to evaluate the wealth effects of corporate litigation: defendants experience economically meaningful and statistically significant wealth losses upon the filing of the suit, whereas plaintiff firms experience no significant wealth effects upon filing a lawsuit. Also, there is a significant wealth increase for defendant firms when they settle a suit with another *firm*, in contrast to other types of plaintiffs, and in contrast to the settling plaintiff firms. These findings suggest that, at a minimum, lawsuits are not a value-enhancing way for corporations to settle their disagreements with other corporations. In addition, the market appears to impose a higher sanction on firms than actual criminal sanctions, and reputational losses are of equal magnitude for civil fines as for criminal ones. The article concludes with some recommendations for researchers: the standards for conducting an event study are well established; researchers can increase the power of an event study by increasing the sample size,

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and by narrowing the public announcement period to as short a time frame as possible. The companion article reviews the use of event studies in corporate law and regulation.

1. Introduction

Event studies are among the most successful uses of econometrics in policy analysis. The methodology, which studies the movement of stock prices due to specific events (unexpected actions by managers or policy makers that are expected to affect firm values) was originally developed to test the hypothesis that the stock market was efficient—that publicly available information is impounded immediately into stock prices such that an investor cannot earn abnormal profits by trading on the information after its release. As evidence accumulated that the stock market was efficient, the methodology came to be used instead to value the event under study. It is through this latter usage that event studies have influenced policy analysis, particularly in corporate and securities law. This is no doubt because there is a natural fit between the methodology and those fields of law: the benchmark for evaluating the benefit of corporate and securities laws is whether they improve investor welfare, and this can be ascertained by what event studies measure, whether stock prices have been positively affected.

The event study methodology is well accepted and extensively used in finance. Event study results have been used in several hundred scholarly articles in leading academic finance journals to analyze corporate finance issues, such as stock repurchases and stock splits and the relation between stock prices and accounting information, by examining the impact of earnings releases. Its use in policy analysis in recent years has become more widespread, and it is the interaction between law and financial econometrics that is the focus of this review.

This is the first of a set of two articles. This one begins by briefly reviewing the event study methodology and its strengths and limitations for policy analysis. It then reviews in detail how event studies have been used to evaluate the wealth effects of one broad area of public policy: corporate litigation. The article concludes with a summary and our recommendations for use of event study results in policy analysis. The second

article, Bhagat and Romano (2002) focuses on the use of event studies in another broad area of public policy: corporate law and corporate governance.

2. A Guide to Event Studies

The price of a stock reflects the time- and risk-discounted present value of all future cash flows that are expected to accrue to the holder of that stock. According to the semistrong version of the efficient market hypothesis, all publicly available information is reflected completely and in an unbiased manner in the price of the stock, such that it is not possible to earn economic profits on the basis of this information.[1] Therefore, only an unanticipated event can change the price of a stock. This change should equal the expected changes in the future cash flows of the firm or the riskiness of these cash flows. Thus, an event is said to have an impact on the financial performance of a firm if it produces an abnormal movement in the price of the stock. Broad stock market movements are usually subtracted from the stock's price movement in estimating the abnormal return. Event studies apply conventional econometric techniques to measure the effect of specific events, such as actions by firms, legislatures, and government agencies, on the stock price of affected firms. Their special use for policy analysis is that they provide an anchor for determining value, which eliminates reliance on ad hoc judgments about the impact of specific events or policies on stock prices.

2.1. Mechanics of Event Studies

An event study has four components: defining the event and announcement day(s), measuring the stock's return during the announcement period,

1. The efficient-market hypothesis has been subjected to extensive empirical testing; perhaps the most intensive and extensive testing of any hypothesis in all of the social sciences. Most tests find evidence consistent with the efficient-market hypothesis. Some studies find that the stock price responds within minutes of a corporate announcement such as a stock offering (see Barclay and Litzenberger, 1988). Most finance scholars hold the view that the stock market in the United States is semistrong form efficient (Welch 2000). But controversy regarding the efficient-market hypothesis lingers. This controversy is based on issues regarding the definition and measurement of risk, and the relationship between risk and return. There is, however, agreement that these issues do not invalidate the event study methodology; see Brown and Warner (1985) and Fama (1990).

estimating the expected return of the stock during this announcement period in the absence of the announcement, and computing the abnormal return (actual return minus expected return) and measuring its statistical and economic significance.

In order to conduct an event study, the researcher first defines the event under investigation. Events are usually announcements of various corporate, legal, or regulatory action or proposed action. Examples of events that have been studied are takeovers, equity offerings, change in state of incorporation, adoption of antitakeover provisions, filing of lawsuits against corporations, deaths of corporate executives, and product recalls. After defining the event the researcher searches for the first public announcement of the event. Identification of the first public announcement of the event is critical since, under the semistrong form of the efficient-market hypothesis, the impact of the event on the value of the firm would occur on the announcement date. Historically, the *Wall Street Journal Index* has been a popular source for announcement dates. More recently, computer accessible databases such as *Lexis-Nexis* and the *Thompson Financial Securities Data* are being increasingly used.

Conceptually, the announcement date is straightforward: it is the "day" the public is first informed of the event.[2] However, identification of this date can sometimes be nontrivial. Consider the announcement of a tender offer. It is possible and probable that news of the tender offer may have leaked to some market participants before the first public announcement. If such is the case then some impact of the tender offer on the firm's share price would occur before the public announcement. Some researchers have attempted to address this issue by considering the period several weeks (or months) before and through the announcement day as the announcement period. However, this obvious solution has two problems, one conceptual and the other technical. Conceptually, it is unclear whether the leakage occurs over a few days, weeks, or months. Technically, as we increase the length of the announcement period, the noise-to-signal ratio increases,

2. Currently, most event studies consider daily returns; hence the announcement period is typically a day. However, historically, some event studies have considered monthly returns—where the announcement need be identified for only a particular month; see the classic study by Fama et al. (1969). More recently, announcements have been identified to the nearest minute, and returns have been computed over minute and trade intervals such that the event study is conducted with intraday data; see Barclay and Litzenberger (1988).

and it becomes increasingly difficult to measure the impact of the tender offer on share price with precision; we will discuss this later in the article. Aside from news-leakage issues, at the time the tender offer is announced there is uncertainty over whether it will be successful, and if successful, over the terms of the final offer. Sometimes the final resolution may not be known for months or even years.

Finally, some events may have several distinct event dates. For example, the enactment of a statute involves many different events, each of which may provide new information to investors regarding the likelihood of passage: when a bill is introduced, when a committee holds hearings on the bill, when one legislative chamber votes on the bill, when a conference committee approves a final bill, and when the executive signs the bill (if there is uncertainty over whether or not the bill will be vetoed). In this context, rather than treat the entire interval from bill introduction to executive signature as the event and run into the problems discussed, the researcher can adapt the methodology to permit each event date to be identified separately; however, in doing so the researcher's bias and priors on what is a significant or relevant event enter the analysis.

After defining the event and announcement period, one measures stock returns for this period. If daily data are being used, this is straightforward: the return is measured through closing prices. Often there is uncertainty if the announcement is made before or after the close of trade on the exchange. To address this, the returns from the next day are often included.

Calculation of the third component is more complicated. Although it is straightforward to measure the actual return for the announcement period, determination of the impact of the event itself on the share price is less so. To measure this impact, the *expected return* must be subtracted from the actual announcement-period return. This expected return is the return that would have accrued to the shareholders in the absence of this or any other unusual event. The finance literature has considered several models of expected returns. These models can broadly be classified as statistical models or economic models.

Statistical models. The constant expected returns model is

$$R_{it} = \mu_i + e_{it}, \tag{1}$$

where R_{it} is the return for stock i over time period t, μ_i is the expected return for stock i, and e_{it} is the usual statistical error term.

The market model is

$$R_{it} = a_i + b_i^* R_{mt} + e_{it}, \tag{2}$$

where, a_i and b_i are firm-specific parameters, and R_{mt} is the market return for the period t.

Economic models. Capital Asset Pricing Model (CAPM) is

$$R_{it} = R_f + \beta_i^*(R_{mt} - R_f) + e_{it}, \tag{3}$$

where R_f is the risk-free rate and β_i is the beta or systematic risk of stock i.

Arbitrage Pricing Theory is

$$R_{it} = \delta_0 + \delta_{i1} F_{1t} + \delta_{i2} F_{2t} + \cdots + \delta_{in} F_{nt} + e_{it} \tag{4}$$

where $F_1, F_2, \ldots, F_n$ are the returns on the n factors that generate returns, and δ are the factor loadings.

The statistical models are simple models of price formation that are not grounded in a specific economic theory. The economic models are derived from specific economic theories of asset price formation. One can think of the economic models as placing certain restrictions on the statistical models (that is, on the slopes and intercepts being estimated).

The choice of a benchmark model can impact both the variance and mean of the abnormal returns. Simulations using actual returns suggest that abnormal returns estimated using statistical models as benchmark are better specified; see Brown and Warner (1985). In addition, since several studies have found evidence inconsistent with the economic models, in particular CAPM, the use of such restrictions is not appropriate. Hence, most researchers have begun to rely on the statistical models to estimate the expected returns during the announcement period. Researchers usually estimate these statistical models by using between 100 and 200 daily returns in the period preceding the announcement period. The unexpected announcement period return, also known as the *abnormal return*, is computed as the actual return minus the estimated expected return. This abnormal return is the estimated impact of the event on the share value.

The fourth and final step is to compute the statistical significance of this abnormal return. The standard error of the residuals from the estimated statistical model can be used as an estimate of the standard error

for the announcement-period abnormal return. However, since individual stock returns are quite volatile, this standard error can be quite high relative to the abnormal return. Event studies usually consider a sample of firms that have made or been the subject of the same type of announcement; each firm's announcement typically has been made on a different calendar day. Another benefit of this approach is that it increases the likelihood that no other information besides the event under study will be valued, since any additional unexpected information disclosed on one firm's announcement date will wash out with that on other firms' announcement days.

The abnormal returns of this sample of firms are averaged to obtain the *average abnormal return.* This average abnormal return is the estimated impact of the event on the share value. Next, the residuals from the estimated statistical model for these firms are averaged in *event time*. Usually the announcement day is defined as *event day 0*. Likewise, t days before (after) the announcement day is defined as *event day* $-t$ (*event day* $+t$). Finally, the standard error of these averaged residuals is used as an estimate of the standard error of the average abnormal return. Under the null hypothesis that the event under study has no impact on firm value, the expected average abnormal return is zero. Additionally, assuming that the announcement period returns for the sample firms are independently and identically distributed, then by the central limit theorem the average abnormal return is normally distributed with mean zero.

The above estimate of the standard error of the average abnormal return would be appropriate if the announcement period abnormal return had the same variance as the estimation period residuals. However, substantial evidence in the finance literature suggests that stock returns in the announcement period are typically more volatile. Brown and Warner (1985) have suggested the use of cross-sectional test statistics when there is an increase in return variance during the announcement period. The standard error of the announcement-period returns for the sample firms is used as an estimate of the standard error of the average abnormal return. Nonparametric tests, such as the Fisher sign test and the Wilcoxon signed rank test, are also conducted on the announcement-period returns; the usual null hypothesis is that the median announcement-period return is zero.

2.2. Statistical Power of Event Studies

If an event changes firm value by a specific amount, say, 1%, can the event study technique detect it with some statistical precision? Equally important, from a statistical, financial, and legal viewpoint is the following question: if an event has no impact on firm value, that is, the announcement-period abnormal return is zero, can the event study technique provide this inference with some statistical precision? These questions can be addressed by considering the statistical power of event studies.

The power of a test statistic is considered in the context of a null hypothesis and an alternate hypothesis. (Hopefully, the alternate hypothesis would be economically meaningful.) In the context of event studies, the usual null hypothesis is that the event has no impact on firm value. An interesting alternate hypothesis could be that the event increases firm value by 1%. Under the assumption that the alternate hypothesis is true, the power of the event study in this context is the probability of observing a statistically significant test statistic. Brown and Warner (1985) and MacKinlay (1997) have studied the power of test statistics typically used in event studies. These authors show that the power of the event study technique improves as the number of firms in the sample increase, as the number of days in the announcement window decrease, and as the alternative of a larger abnormal return is considered against the null hypothesis of zero abnormal return.

The following numerical examples from MacKinlay (1997, Table 2) illustrate the power of the event test methodology and how the power can be enhanced.

For a one-day announcement window, a sample size of 25 firms, and a two-sided test with a 5% significance level, the probabilities of detecting an abnormal return of 0.5%, 1%, and 2%, are 24%, 71% and 100%, respectively.

- If the sample size were increased to 50 firms, the probabilities of detecting an abnormal return of 0.5%, 1%, and 2%, are 42%, 94%, and 100%, respectively.
- If the sample size were increased to 100 firms, the probabilities of detecting an abnormal return of 0.5%, 1%, and 2%, are 71%, 100%, and 100%, respectively.

• For a two-days announcement window (or equivalently, doubling of the standard deviation of the event-day abnormal return), and a sample size of 25 firms, the probabilities of detecting an abnormal return of 0.5%, 1%, and 2% are 10%, 24% and 71%, respectively.

• For this two-days announcement window and a sample size of 50 firms, the probabilities of detecting an abnormal return of 0.5%, 1%, and 2% are 14%, 42%, and 94%, respectively.

• For this two-days announcement window and a sample size of 100 firms, the probabilities of detecting an abnormal return of 0.5%, 1%, and 2% are 24%, 71%, and 100%, respectively.

The above findings suggest that the power of the event study diminishes as the sample size decreases. An important question is, can an event study be conducted with just one firm, that is, is a sample size of one acceptable? This question is especially relevant in court cases or regulatory injunctions involving only one firm. Conceptually, a sample of one is a rather small sample, but this by itself does not invalidate the event study methodology. However, the statistical power with a sample of one is likely to be quite low. First, the variability of (abnormal) returns of a portfolio with just one stock in it is significantly higher than a portfolio with even a few, say five, stocks in it. Any standard finance or investment textbook will have a graph depicting the sharp drop in variance of portfolio returns as the number of stocks in the portfolio increases from one, to five, to ten; after about 50 stocks in the portfolio, the decrease in variance is quite small. Second, it is plausible that the announcement-period return of an announcing firm will be affected by other information unrelated to the event under study. If a sample of one is considered, it is quite difficult to determine the separate effects on firm value of the announcement and of the unrelated information item(s). If the sample has several firms, then the effect on firm value of such unrelated information is likely to cancel out. As the sample size increases, the effect on firm value of such unrelated information (goes to zero) becomes less and less significant.

The above findings also suggest that the power of the event study methodology diminishes substantially as the event period is increased from one to just two days. During the past decade an increasing number of finance studies have considered abnormal returns for long-horizon

windows of several *years*. Such studies have considered abnormal returns over *twelve to sixty months* after the announcements of various corporate events like mergers, share repurchases, initial public and seasoned equity offerings, spinoffs, stock splits, and dividends. Examples of such studies include Ikenberry, Lakonishok, and Vermaelen (1995); Loughran and Ritter (1995); Brav and Gompers (1997); McConnell, Ozbilgin, and Wahal (1999); Desai and Jain (1999).

There are two reasons for studying the long-horizon window of several years after an announcement. First, the market may be unable to fully understand and incorporate the impact of the announcement on the company's value. Over time the market gets the opportunity to fully understand and incorporate the impact of the announcement on the company's value. Under this explanation, no new information related to the first announcement is released in this postannouncement period; hence this reason presumes a semistrong form *inefficient* market. Second, new information pertinent to the initial announcement may become known to the market participants in the months or years subsequent to the announcement. For example, the initial announcement could be a takeover offer announcement. Before the offer is finalized and completed several events could occur that might change the likelihood of the success of the initial offer. Examples of such events include the arrival of a second bidder, litigation by target management, and regulatory objections (see Bhagat, Hirshleifer, and Noah, 2001). In this scenario, one way to estimate the full impact of the initial event would be to consider the period from the initial announcement through final resolution—a period that could extend several years in some cases.

Kothari and Warner (1997), Barber and Lyon (1997), and Lyon, Barber, and Tsai (1999) have raised serious concerns about the specification and power of the event study methodology when long-horizon windows of several years are considered. Kothari and Warner find that the event study test-statistics used in the above-mentioned studies are generally misspecified in the sense that they reject the null hypothesis of normal performance when there is no abnormal performance too frequently, given the significance level. Lyon, Barber, and Tsai suggest ways to construct properly specified test-statistics. However, these authors caution that though these test statistics appear to be well specified for random samples, they are not well specified for nonrandom samples. Given that tests of most interest-

ing finance and legal hypotheses are likely to lead to the construction of nonrandom samples, the concern with the misspecification of the long-run test-statistics remains. Finally, Lyon, Barber, and Tsai document the power of the long-horizon test statistic to detect abnormal performance when it is actually present. Using state-of-the-art techniques, for a twelve-month buy-and-hold abnormal return,[3] a sample size of 200 firms, and a one-sided test with a 5% significance level, the probabilities of detecting an abnormal return of 5%, 10%, and 20%, are 20%, 55% and 100%, respectively. As the horizon increases beyond twelve months, and the sample size decreases, the power of the technique would further diminish. For these reasons, these authors conclude that "the analysis of long-run abnormal returns is treacherous."

2.3. Cross-Sectional Determinants of the Stock Market's Reaction

Some researchers have sought to provide insight into the cross-sectional determinants of the stock market's reaction to the announcement of an event by examining the relation between the size of the abnormal return (AR) identified in an event study and characteristics specific to the event observations, that is, cross-sectional differences in the firms in the study. This approach can be used, for instance, where there are multiple hypotheses for the source of a wealth effect. The AR is the dependent variable in an ordinary least squares regression on the firm characteristics of interest:

$$AR_j = d_0 + d_1 x_{1j} + \cdots + d_M x_{Mj} + e_j, \tag{5}$$

where AR_j is the j^{th} abnormal return observation, x_{mj}, $m = 1, \ldots, M$, are M characteristics for the j^{th} observation, and e_j is the zero mean disturbance term that is uncorrelated with the x's. Additionally, d_m, $m = 0, \ldots, M$ are the regression coefficients.

3. Buy and hold returns mimic the returns of an investor that buys and holds the portfolio for the entire period under study; usually an equal amount is assumed to be invested in each of the stocks in the portfolio and no rebalancing of the portfolio is done. Cumulative abnormal returns (abnormal returns summed or cumulated over the days under study) assume that the portfolio is rebalanced daily such that an equal amount is assumed to be invested in each of the stocks in the portfolio at the start of each day. Mathematically, buy and hold returns are computed as products rather than sums.

This approach has been used in a variety of contexts. We note here an illustration from the methodology's application to assessing the wealth effects of corporate litigation discussed in section 3. Bhagat, Brickley, and Coles (1994) provide an example of its use in determining the source of the significant negative wealth effects experienced by corporate defendants. They find that the negative abnormal returns from litigation are significantly related to variables proxying for the defendant's proximity to financial distress.

An interpretational concern involving cross-sectional models is whether the abnormal return is related to the firm characteristics not only through the wealth effect identified in the event study but also through investors' anticipation of the event. Namely, investors may expect that firms with the specified characteristics will be subject to the event under study. In this case the linear specification will not uncover a relation between the variables. Moreover, the greater the connection between the specified characteristics and the occurrence of the event—that is, the more highly the event is anticipated—the less likely a relation will be found in the cross section because the information effect (the AR) will be that much smaller (Bhagat and Jefferis, 1991; Prabhala, 1997). MacKinlay (1997) provides an overview and further references. The issue also implicates event studies in general, for if the anticipation is sufficiently great, there will be no announcement effect; given this possibility, some researchers have proposed the use of a conditional approach instead of the conventional approach that we have discussed (for example, Acharya 1988). However, Prabhala (1997) shows that the significance test for the existence of an information effect in the traditional methodology is, in fact, well specified. He also shows the circumstances under which the regression coefficients on firm characteristics in traditional cross-sectional models are proportional to the true cross-sectional parameters, and hence the associated t-statistics may be interpreted as a conservative (lower bound) estimate of the parameters' true statistical significance. We therefore conclude that the principal use of cross-sectional models will continue to be for refinement of researchers' theories for undertaking their event studies by explaining the results of the standard model—that is, for relating the size and sign of the abnormal returns to specified firm and event characteristics.

3. Shareholder Wealth Implications of Corporate Lawsuits

In the 1980s and 1990s business frequently complained about a litigation explosion and the costs associated with legal disputes, raising concerns that the U.S. legal system affected firms' competitiveness in global markets. Surveying corporate legal department budgets, Economic Analysis Group, Craig Consulting Company, and Endispute estimated that salaries to in-house lawyers and fees to outside counsel for the 1,000 largest public companies hit $20 billion in 1991.[4] Large liability or settlement payments undoubtedly dwarf direct legal costs. Indeed, some mass torts, such as the breast-implant cases against Dow Corning and the Dalkon Shield cases against A. H. Robins, have threatened the existence of defendant firms, forcing them into insolvency proceedings.

It is, however, possible that estimates of business' legal costs are overstated, reflecting political agendas or overreaction to media coverage of a few spectacular cases. Many large publicized damage awards, for example, are overturned on appeal or significantly reduced in a settlement (Shanley and Peterson, 1987). In addition, much corporate litigation involves contract disputes between firms.[5] But concerns over litigation have continued into the 1990s: tort reform was one of ten points in the Republican party's "Contract with America" 1994 campaign platform, under which it gained a majority in the House of Representatives for the first time in 40 years, and successful litigation initiatives against tobacco companies that produced a settlement of over $200 billion have led to other industry targets, such as health care providers.

Event studies can be used to identify and measure the costs of lawsuits against firms, and they have been particularly used to evaluate the costs of interfirm litigation. The results are quite uniform: when the costs and benefits to both parties are computed, litigation is not a positive net present value event for both firms considered together. This result is not surprising:

4. An article in Forbes, citing statistics from a RAND study on tort litigation, estimated the direct costs of all lawsuits, including those involving business, to be as high as $117 billion a year (Spencer, 1992, p. 40). Another estimate (p. 41) placed litigation costs as high as 2.5% of GNP.

5. For example, a RAND study of Fortune 1,000 companies found that contract disputes between firms constituted the largest single category of federal civil suits (Dungworth and Pace, 1990).

it is an impetus motivating the successful move to greater use of alternative dispute resolution, particularly in the corporate context.

3.1. Wealth Effects of Corporate Litigation

The primary focus in the literature has been on "leakages" in the litigation process: negative wealth effects upon netting the parties' gains and losses. For example, Cutler and Summers (1988) examine the Pennzoil-Texaco lawsuit, which involved a claim of tortious interference of a merger contract, and find significant costs to both parties from the dispute, with the losses for the losing defendant Texaco being larger than the gains for the winning plaintiff Pennzoil. The combined drop in value for the two firms was $2 billion. They attribute the loss mainly to an increase in the probability of financial distress for Texaco. Engelmann and Cornell (1988) study the wealth implications around filings, settlements, and verdicts for a sample of five interfirm disputes. They too observe combined wealth losses, or leakages, to the litigating parties. Bhagat, Brickley, and Coles (1994) examine the market reaction to lawsuit filings and settlements for a much larger sample of 550 interfirm disputes. They observe combined wealth losses arising from lawsuit filings and find that these leakages are a result of increased probability of financial distress for the defendant. In addition, they find that defendant firms gain upon the announcement of a settlement.

Ellert (1975) examines the market responses to announcements of legal challenges to mergers under Section 7 of the Clayton Act by the Federal Trade Commission and Department of Justice over the period 1950–1972. During the month of the announcement of the suit, the market adjusts defendant firm value downward by about 2%. Bizjak and Coles (1995) analyze a more homogeneous but still large sample of interfirm disputes—private antitrust suits. To our knowledge, this is the only study to find a positive stock market reaction to plaintiffs upon any sort of lawsuit filing. They also find that the joint wealth effects associated with the announcement of a filing tend to be negative and that leakages in antitrust disputes are attributable to court-imposed behavioral restraints, the likelihood of follow-on suits, and an increased likelihood of financial distress. Moreover, they confirm that factors which affect the costs of litigation also affect behavior in suit, settlement, and trial. In their sample of antitrust lawsuits, the parties are more likely to settle when the suit involves poten-

tial restrictions on the defendant's business practices and when there is the potential for financial distress.

Event studies have also been used to address the validity of the government's antitrust actions against various corporations. The argument goes that for a corporation exercising market power, the government's antitrust action against it will lower its share price *and* increase the share price of its competitors. The competitors will experience a positive reaction since the government's antitrust action increases the odds that these competitors will be competing in an industry without a dominant company that might be exercising market power. Bittlingmayer and Hazlett (2000) use this intuition to evaluate the U.S. Department of Justice's recent antitrust action against Microsoft. They find evidence inconsistent with the joint hypothesis that Microsoft's behavior has been anticompetitive and that the antitrust enforcement enhances economic efficiency.

Finally, Bhagat, Bizjak, and Coles (1998) analyze a large sample of lawsuits in which at least one side, plaintiff or defendant, is a corporation. To estimate the implications of litigation for shareholder wealth, they examine the abnormal stock market reaction to filing and settlement announcements. They find that the average wealth loss for a defendant is 0.97% of the market value of the equity, or $15.96 million. They further test whether characteristics of the suit, such as legal issue, type of opponent, and firm characteristics (such as firm size and proximity to bankruptcy) have power to explain cross-sectional variation in these wealth effects.

Bhagat, Bizjak, and Coles find that no matter who brings a lawsuit against a firm, be it a government entity, another firm, or private citizen, defendants experience economically meaningful and statistically significant wealth losses upon the filing of the suit. Furthermore, they find some evidence that the identity of the plaintiff has an influence on the wealth effects upon filing. Defendants involved in government suits suffer larger declines in shareholder wealth (−1.73%) than defendants involved in lawsuits with other firms (−0.75%) or with private parties (−0.81%). This result is consistent with the notion that government agencies have more leverage and resources at their disposal to use in a legal battle or the type of suit most frequently filed by government agencies, such as an environmental action, is typically more serious—or both. Indeed, they do find that certain types of litigation are more costly for defendants. Environ-

mental suits (−3.08%), product liability suits (−1.46%), and violations of security laws (−2.71%) result in significantly greater wealth losses for defendant firms, compared to disputes involving antitrust or breach of contract issues. It appears that, at least for some types of suits, the actual or potential lawsuit is associated with a large decline in shareholder wealth and a corresponding nontrivial deterrent effect. The results of these and other studies that consider the impact of litigation on corporate value are summarized in Tables 1–3.

Bhagat, Bizjak, and Coles also find that the defendant wealth effect on announcement of a filing is significantly positively related to the size of the firm and, in some specifications, significantly negatively related to the firm's proximity to bankruptcy. One possible explanation for this effect of firm size is that larger firms can have more bargaining power or more resources to devote to the legal dispute (e.g., because of better access to capital markets or "deep pockets"). The results on proximity to bankruptcy are consistent with other work that has identified potential bankruptcy costs as an important indirect cost of a legal dispute (Bhagat, Brickley, and Coles, 1994; Bizjak and Coles, 1995; and Cutler and Summers, 1988).

For plaintiff firms, Bhagat, Bizjak, and Coles find no significant wealth effects associated with lawsuit filings. They also find that the identity of the defendant—that is, whether the defendant is another firm, a government agent, or private citizen—and the legal issue are not related to the stock price change of the plaintiff when a suit is filed. They are, accordingly, unable to detect in the data evidence of strong incentives for plaintiffs to sue.

Bhagat, Bizjak, and Coles' results indicate that when a defendant firm settles a suit with another firm there is a significant wealth increase. It is surprising that, in contrast, they can detect no significant wealth change for defendants upon announcement of a settlement when the opponent is a governmental entity or noncorporate private party. In addition, the wealth effect of a settlement for the defendant is unrelated to the legal issue. For plaintiff firms the wealth implications of settlements appear to be trivial. On average, they find no significant wealth gains or losses to plaintiff firms who settle a lawsuit, and neither legal issue nor the identity of the opposing party has power to explain variation in those returns. These data suggest that lawsuits are not positive net present value undertakings for plaintiffs, since the absence of positive abnormal returns on settlement

Table 1. Announcement-Period Abnormal Returns for Defendant Corporations by Opponent Type

Plaintiff	Study	Sample Period	Sample Size	Announcement Window: (Event Days)	Announcement Return (%)	*Z*-statistic
Another firm	BBC (1998)	1981–1983	239	Filing (−1,0)	−0.75**	−3.31
Government	BBC (1998)	1981–1983	110	Filing (−1,0)	−1.73**	−4.99
Private nonfirm	BBC (1998)	1981–1983	221	Filing (−1,0)	−0.81**	−2.67
Another firm	BC (1995)	1973–1983	343	Filing (−1,0)	−0.60**	−3.17
Stakeholders	KL (1993)	1978–1987	19	Allegation (−1,0)	−1.34	−1.21
Stakeholders	KL (1993)	1978–1987	25	Filing (−1,0)	−1.67*	−2.35
Government	KL (1993)	1978–1987	13	Allegation (−1,0)	−5.05**	−4.77
Government	KL (1993)	1978–1987	17	Filing (−1,0)	−0.93	−1.14
Stakeholders	KL (1999)	1979–1995	80	Filing (−1,0)	−1.02**	−2.86
Consumers	PR (2002)	1985–1995	15	Filing (−1,1)	−1.93**	−3.31
Another firm	BBC (1998)	1981–1983	12	Settlement (−1,0)	3.66**	3.29
Government	BBC (1998)	1981–1983	4	Settlement (−1,0)	−0.68	−0.22
Private nonfirm	BBC (1998)	1981–1983	12	Settlement (−1,0)	−1.06	−1.72
Stakeholders	KL (1993)	1978–1987	13	Settle/Verdict (−1,0)	−0.17	−0.49
Government	KL (1993)	1978–1987	10	Settle/Verdict (−1,0)	1.48	1.20
Stakeholders	KL (1999)	1979–1995	15	Verdict-Defense (−1,0)	−0.36	−0.51
Stakeholders	KL (1999)	1979–1995	193	Verdict-Plaintiff (−1,0)	−0.62*	−2.74
Stakeholders	KL (1999)	1979–1995	4	Settlement (−1,0)	−2.43	−1.35
Consumers	PR (2002)	1985–1995	25	Verdict-Plaintiff (−1,1)	0.33	0.73

Notes: Event day 0 is the publication date of the filing, allegation, or settlement. BBC (1998): Bhagat, Bizjak, and Coles (1998). BC (1995): Bizjak and Coles (1995). KL (1993): Karpoff and Lott (1993). KL (1999): Karpoff and Lott (1999). PR (2002): Prince and Rubin (2002).

*Significant at the 95% confidence level, two-tailed test.

**Significant at the 99% confidence level, two-tailed test.

Table 2. Announcement-Period Abnormal Returns for Plaintiff Corporations by Opponent Type

Defendant	Study	Sample Period	Sample Size	Announcement Window: (Event Days)	Announcement Return (%)	Z-statistic
Another firm	BBC (1998)	1981–1983	172	Filing (−1,0)	−0.25	−0.60
Government	BBC (1998)	1981–1983	26	Filing (−1,0)	−0.44	−0.80
Private nonfirm	BBC (1998)	1981–1983	51	Filing (−1,0)	0.71	0.34
Another firm	BC (1995)	1973–1983	86	Filing (−1,0)	1.24**	4.26
Another firm	BBC (1998)	1981–1983	8	Settlement (−1,0)	−0.77	−1.26

Notes: BBC (1998): Bhagat, Bizjak, and Coles (1998). BC (1995): Bizjak and Coles (1995).

**Significant at the 99% confidence level, two-tailed test.

Table 3. Announcement-Period Abnormal Returns for Defendant Corporations by Type of Legal Issue

Legal Issue	Study	Sample Period	Sample Size	Announcement Window: (Event Days)	Announcement Return (%)	Z-statistic
Antitrust	BBC (1998)	1981–1983	62	Filing $(-1, 0)$	−0.81	−1.52
Breach of contract	BBC (1998)	1981–1983	48	Filing $(-1, 0)$	−0.16	−0.59
Corp. governance	BBC (1998)	1981–1983	154	Filing $(-1, 0)$	0.08	0.64
Environment	BBC (1998)	1981–1983	27	Filing $(-1, 0)$	−3.08**	−5.32
Exclusive dealing	BBC (1998)	1981–1983	27	Filing $(-1, 0)$	−0.14	0.28
Patent infringement	BBC (1998)	1981–1983	33	Filing $(-1, 0)$	−1.50*	−2.42
Product liability	BBC (1998)	1981–1983	38	Filing $(-1, 0)$	−1.46**	−3.12
Disclosure laws	BBC (1998)	1981–1983	46	Filing $(-1, 0)$	−2.71**	−4.49
Antitrust-horizontal	BC (1995)	1973–1983	117	Filing $(-1, 0)$	−1.45**	−4.88
Antitrust-vertical	BC (1995)	1973–1983	105	Filing $(-1, 0)$	0.27	1.29
Fraud of stakeholders	KL (1993)	1978–1987	19	Allegation $(-1, 0)$	−1.34	−1.21
Fraud of stakeholders	KL (1993)	1978–1987	25	Filing $(-1, 0)$	−1.67*	−2.35
Fraud of government	KL (1993)	1978–1987	13	Allegation $(-1, 0)$	−5.05**	−4.77
Fraud of government	KL (1993)	1978–1987	17	Filing $(-1, 0)$	−0.93	−1.14
Financial reporting fraud	KL (1993)	1978–1987	4	Allegation $(-1, 0)$	−4.60*	−2.00
Financial reporting fraud	KL (1993)	1978–1987	7	Filing $(-1, 0)$	−4.56*	−1.99
Punitive damages	KL (1999)	1979–1995	80	Filing $(-1, 0)$	−1.02*	−2.86
Product liability	PR (2002)	1985–1995	15	Filing $(-1, 1)$	−1.93**	−3.31

Notes: Event day 0 is the publication date of the filing, allegation, or settlement. BBC (1998): Bhagat, Bizjak, and Coles (1998). BC (1995): Bizjak and Coles (1995). Horizontal antitrust issues include horizontal price fixing, merger/joint-venture, asset accumulation, predatory pricing, and monopolization. Vertical antitrust issues include resale price maintenance, exclusive dealing, tying, territorial restrictions, dealer termination, and refusal to deal. KL (1993): Karpoff and Lott (1993). Fraud of stakeholders occurs when the firm is accused of cheating on implicit or explicit contracts with suppliers, customers, or employees. Fraud of government occurs when the firm is accused of cheating on implicit or explicit contracts with government agencies. Financial reporting fraud occurs when the firm is accused of misrepresenting the firm's financial condition. KL (1999): Karpoff and Lott (1999). Punitive damages are sought in cases involving product liability, fraud, business negligence, breach of contract, insurance claims, employment claims, asbestos claims, and vehicular accident claims. PR (2002): Prince and Rubin (2002). Product liability claims involving auto manufacturers.

*Significant at the 95% confidence level, two-tailed test.

**Significant at the 99% confidence level, two-tailed test.

cannot be explained by investor anticipation upon the lawsuit filing (there was no significant positive gain at the earlier date).

3.2. The Effect of Litigation on the Value of Brand Name, Trademarks, and Corporate Reputations

Klein and Leffler (1981) argue that a company's investments in brand names and trademarks provide implicit guarantees to consumers of quality products. These authors posit that consumers are willing to pay a premium for branded and trademarked products. They suggest that firms and consumers have an implicit contract—firms that produce higher-quality products have consumers who are willing to pay high prices. If the firm reneges and fails to maintain consistent quality, consumers do not repeat-purchase, and the company will not be able to recoup its investment in the brand name or trademark.

One event study has indicated that the value of a brand name is, indeed, substantial and related to quality assurance: Mitchell (1989) studied the stock price effect of Johnson & Johnson's recall of its Tylenol capsules after serious product tampering and found that by far most of the loss ($1.24 billion of $1.44 billion, a loss estimation based on the stock's relationship with the over-the-counter drug market, whose firms also were negatively impacted by the incident) represented a decline in the value of the brand name of the firm and the product, as out-of-pocket costs of the recall were about $200 million. This was a 14.3% decline in its stock price relative to its forecasted value. Although Tylenol's market share eventually recovered to close to pretampering levels, it never reached the level that was forecasted before the event, the sale of Tylenol tablets declined even though they had not been subject to the tampering, and the company delayed the introduction of new drugs.

Landes and Posner (1987) suggest a framework of company-consumer interaction that makes a brand name or trademark valuable to both consumers and corporations. First, consumers value a trademark because it reduces their mental and time costs of identifying and describing the product they want. Second, trademarks help reduce search costs for consumers. Suppose a consumer had a positive experience with a prior purchase of a brand or was recommended a brand by a friend who was satisfied from the use of the brand. The consumer would like to purchase that brand again with a minimum of effort searching for it. In the absence of a brand

name or trademark, the brand can be found by evaluating all attributes or searching among numerous brands. In contrast, search costs for the consumer are much lower when she is looking for a specific brand name or trademark and then purchases the brand.

Third, the consumer will find it worthwhile to search for the branded product that previously provided a positive experience, if the prior experience is a good predictor of future experience. In other words, the consumer will look for a trademarked brand only if it is of consistently high quality. Consumers cannot be repeatedly fooled about the quality, for instance, by false advertising about the quality of attributes. If the brand's quality is inconsistent, the consumer will not be able to use the brand name or trademark to relate past experience to future consumption experiences. The brand name or trademark will not lower search costs, because the consumer will have to search and evaluate attributes and brands. Consequently, the consumer will be unwilling to pay more for the branded product over the unbranded product. Firms will not be able to charge high prices for brands that have substandard quality. Hence, a firm that produces a brand with poor or inconsistent quality will not find a brand name or trademark to have as much value. This suggests that aside from potential bankruptcy costs, court costs, and punitive fines, corporations will incur a reputational cost if a lawsuit has an adverse impact on the value of their brand name and trademark.

The Klein and Leffler model, and its extension by Landes and Posner, would suggest that related-party corporate crime, where customers and other related parties may stop dealing with the corporation or otherwise change their willingness to pay for the defendant's product, will impose reputational damages, while third-party offenses will not. Alexander (1999) shows that this is in fact the case: corporations committing related-party crime (i.e., contract fraud) experience significant reputational losses, whereas those committing third-party offenses (i.e. violations of environmental law) do not. She further provides evidence on the reason for the observed reputational losses: for example, in 57% of the contract related-party criminal cases, customer dealings were suspended or terminated, whereas this occurred in only 14% of the third-party crime cases.

In a series of papers, Karpoff and Lott (1993, 1999) document the importance of reputational costs imposed on defendant corporations. On the basis of their empirical evidence they argue that criminal restitution,

civil penalties, and court costs comprise only about 7% of the shareholder wealth loss. They argue that the remaining 93% can be attributed to the reputational loss suffered by the defendant firms. The market thus appears to impose significant costs on firms for engaging in criminal conduct. This is also true for firms subject to punitive damage award lawsuits, as the median loss in market value over the announcement period regarding the litigation is far greater than the nominal cost of the awards; but the absolute dollar amount of reputational loss in these cases is about half as large as that for firms involved in criminal or civil fraud lawsuits.

Michael Block (1991) compares the stock price effects of corporate fraud with those of certain crimes referred to as *malum prohibitum* crimes, that is, crimes that have negligible effect on parties in contractual relations with the firm, such as tax evasion, money laundering, and currency-reporting violations. He finds significant negative price effects only for the fraud cases. This suggests that the reputational costs of corporate crime are fairly specific. As Block puts it, "Simple conviction of a criminal act does not generally stigmatize" (p. 414). Block further examines the stock price effects for certain civil fines—federal safety regulation violations by airlines. He again finds a significant negative price effect, of the same magnitude as that experienced by the firms charged with criminal fraud, −2.2%. This suggests that civil enforcement may be equally as effective in imposing reputational penalties as criminal enforcement.

Private civil litigation does not, however, appear to have similar reputational consequences (at least, in the absence of punitive damage awards): Prince and Rubin (2002) examine product liability litigation, and offer data suggesting that the significant negative stock price declines experienced by defendant firms upon lawsuit filing approximate the out-of-pocket costs of the litigation and therefore do not seem to include additional reputation losses. This differs markedly from the impact of government-mandated product recalls: Jarrell and Peltzman (1985), for instance, find that the stock price losses to firms upon the announcement of a recall are substantially greater (as much as ten times) than the out-of-pocket costs. One possible explanation of these disparate results is that the market does not view the filing of a product liability lawsuit, compared to a government recall, as evidence of a defective product that would diminish the value of a corporate brand name or reputation.

4. Summary and Recommendations

4.1. Summary of Wealth Effects of Corporate Lawsuits

Defendants experience economically meaningful and statistically significant wealth losses upon the filing of the suit. Defendants involved in government suits suffer larger declines in shareholder wealth than defendants involved in lawsuits with other firms or with private parties. Plaintiff firms experience no significant wealth effects upon filing a lawsuit. Also, when a defendant firm settles a suit with another *firm* there is a significant wealth increase for the defendant. In contrast, no significant wealth effects are observed for defendants upon announcement of a settlement when the opponent is a governmental entity or noncorporate private party. For plaintiff firms the wealth implications of settlements appear to be trivial. These findings suggest that, at a minimum, lawsuits are not a value-enhancing way for corporations to settle their disagreements with other corporations. Finally, the market appears to impose a higher sanction on firms than actual criminal sanctions, and the reputational losses are of equal magnitude for civil fines as for criminal ones.

Two caveats are in order regarding these findings. First, the announcement-period abnormal return understates the expected decline in shareholder wealth. The reason is that information about the forthcoming suit may already have reached the market (before announcement in the press) and therefore already be reflected in the market price of the firm's stock. Most of the studies have attempted to reduce the severity of this problem by excluding cases where there was indication in published news reports that information about the suit had previously reached the public. Second, event studies of litigation report the average market response associated with the filing or settlement of a lawsuit. Under what circumstance would a court, corporate manager, or corporate legal counsel use such information? Virtually no litigation situation is an average situation. Each suit represents a unique set of costs and benefits, and managers deciding whether to launch or defend a suit will consider the specific costs and benefits of their situation, rather than the average market response to a collection of suits that may or may not share similar characteristics. However, it is precisely information in a wide spectrum of suits that is most useful for the ex ante formulation of public policy and corporate strategy.

4.2. Recommendations for Use of the Event Study Methodology

The standards for conducting an event study are well established. A researcher can increase the power of an event study by increasing the sample size, narrowing the public announcement to as short a time frame as possible, or both.

How large should the sample size be? In general, the larger the better. This said, the recommended sample size would depend on the magnitude of the abnormal return that one is trying to detect. If the abnormal return is about 1% (and the announcement window can be narrowed to one day) then a sample of 100 firms would be sufficient. If the abnormal return is only 0.5% (and the announcement window can be narrowed to one day) then we would recommend a sample of 200 firms. On the other hand, in general, a sample of just one firm would be quite inadequate in detecting an abnormal return of even 2%.

Regarding the length of the announcement window: the shorter the better. If one is using daily return data, an announcement window of one day is quite feasible and the window that we recommend. However, in going from one to two or three days, the loss in statistical power is not serious. But it is very difficult to have much confidence in the results of event studies that consider long-horizon returns of several years.

Many topics of interest to legal researchers involve events that will produce a data set that does not fall into these extreme cases. For instance, if the topic of investigation is the wealth effect of a specific state law, it may be impossible to identify a one-day event interval. Given the nature of the legislative process, statutory changes typically occur over an interval significantly longer than one day, encompassing at least several months. In this setting, the researcher should try to narrow the event interval as best as he or she can: for instance, by examining the impact on returns only of specific event days (introduction of the bill, committee hearing, chamber vote) over the longer legislative interval. But identification of a single event day is not always possible. In addition, the number of firms affected by one state statute is likely to be substantially below 100 in all but a few states. Inability to increase sample size or narrow the event interval does not indicate that the methodology cannot or should not be used: rather, it means that interpretation of results, such as a finding of insignificance, should be undertaken with care. For a sample of

50 firms and an event date consisting of a one-week interval, for example, the event would have to produce an abnormal return of about 4% to be reliably detected, although there may be a further question whether a smaller level of abnormal returns would be considered economically significant.

The event study methodology, accordingly, can be useful to analyze a variety of issues of interest to both lawyers and economists, or more generally, to public policy analysts. In the companion article to this one, we review its extensive use in illuminating the policy debates in corporate law and corporate governance, as well as issues in its application to the study of regulation.

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